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THE LIMITS OF PURE REALISM AND EMPIRICISM IN THEORIES OF INTERNATIONAL RELATIONS

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PURE REALISM AND EMPIRICISM
IN THEORIES OF
INTERNATIONAL RELATIONS

by

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INTRODUCTION

> ... Young sciences, like young men, have their time of wonder, hope, imagination, and of passion too, and haste, and bigotry. Dazzled, and that pardonably, by the beauty of the few laws they have discovered, they are too apt to erect them into gods, and to explain by them all matters in heaven and earth; and apt, too ... to patch them where they are weakest, by that most dangerous succedaneum of vague and grand epithets, which very often contain, each of them, an assumption far more important than the law to which they are tacked.[1]

That this should have been written over a century ago, in an essay on "limits" gives one pause. This is also an essay on "limits," the same type of limits to which Kingsley referred: those which define a science that seeks to answer questions we normally think of as non-scientific. Certainly a century has been adequate to make these limits clear.

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[1] Charles Kingsley, The Limits of Exact Science as Applied to History (London: MacMillan & Co., 1860), p. 17.

Or has it? The past hundred years has seen many changes in the application of scientific method to social theory. A revolution in data handling and data reduction, as well as one in communications, has placed most facts within the confident reach of the social theorist. At the same time, and as scientific theories abound, the critics of scientific method as applied to politics have doubled and redoubled their efforts to show the irrelevance of any theories that might result from such an application. It must be admitted, then, that the limits of science in social theory are not known, or at least are imperfectly known.

The purpose of this thesis is to explore the limits of pure realism and empiricism in theories of international relations, with the object of reaching a decision as to whether it is, or will ever be, possible to construct a science of politics. This will not be the first time the question of the possibility of creating a science of politics has been engaged, as Dr. Kingsley's essay shows. Undoubtedly, it will not be the last, for those who have chosen sides in the debate over science in politics are not likely to give up their views easily.

One purpose which this thesis does <u>not</u> have is to arbitrate the dispute between the realist and the empiricist. There has been a dispute, as Hans Morgenthau's

very title Scientific Man vs Power Politics suggests. It results, quite simply, from the fact that the realist acknowledges finite limits to his understanding and the empiricist does not - or at least does not seem to. The two speak different languages: the realist tends to become a moral philosopher and the empiricist one form or other of manipulator. An accommodation between these two theories is impossible.

The thesis here is that the theories of pure realism, as expounded by Hans J. Morgenthau in his principal work Politics among Nations, and empiricism, as delineated by Morton A. Kaplan in his magnum opus System and Process in International Politics, are equally erroneous and erroneous for very similar reasons. These reasons themselves lead the way to a discovery of their limits as theories of international relations and to a determination as to the practicability of ever attaining a science of politics.

This thesis starts with a critical comparison of the theories of international relations of Morton A. Kaplan and Hans J. Morgenthau. A comparison between Kaplan and Morgenthau which seeks similarities may seem at first a very strange endeavor. After all, Kaplan's theory is admittedly "a systematic effort to cope with the many aspects of international politics from an abstract, theoretical and semiformal point of

view."[2] Of this type of effort, Professor Morgenthau has remarked that:

> ... The retreat into the trivial, the formal, the methodological, the purely theoretical, the remotely historical - in short, the politically irrelevant - is the unmistakable sign of a "non-controversial" political science which has neither friends nor enemies because it has no relevance for the great political issues in which society has a stake.[3]

The juxtaposition of these two excerpts makes it appear that Kaplan and Morgenthau should be archenemies as theorists of international politics. Could it be that they have anything in common?

It is the contention of this writer that Kaplan and Morgenthau have a great deal in common; that their theories spring from a common conceptual root, that they use a similar methodology in their approach to theory-building, and that their theories end in substantially the same prescriptions for resolution of world problems. The "utopias" of both become irrelevant for largely the same reasons. Therefore first part of this thesis explores these similarities in root, method and conclusions, respectively. The likenesses are instructive not for their own sake,

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[2] Morton A. Kaplan, System and Process in International Politics (New York: John Wiley & Sons, Inc., 1957), p. xiii.

[3] Hans J. Morgenthau, Dilemmas of Politics (Chicago: University of Chicago Press, 1958), p. 31.

but because they highlight the basic weakness of the approach to theory-building which they represent.

A critical comparison between Kaplan and Morgenthau is not the purpose of this thesis, however. It can only lead the way to a full discussion of the place and purpose of method in theory and to an answer to the question of the possibility of a real "political science." This discussion is contained in the second part of this thesis. It concludes that a true political science is indeed impossible, for reasons given therein and elucidated in the three-chapter discussion of the Kaplan and Morgenthau theories.

Before beginning the discussion in earnest, two points should be made. First, any critic of Hans J. Morgenthau, and this thesis contains many criticisms of him, should give due credit to the great mind that produced Politics among Nations. It is strange that many otherwise serious students of international relations consider it almost a duty to criticize Morgenthau. To be sure, the theory of pure realism has many limitations. But it is, Kaplan's theory notwithstanding, the most internally consistent conceptual framework produced to date. The student should understand the basic flaw in Politics among Nations is not its wholesale adherence to the theory of power politics but its failure to grasp the significance of man's

ability to transform what Morgenthau himself calls the "struggle for power" into a "struggle for peace." If a reader of this thesis believes that politics is not a struggle for power, he may disagree violently with my conclusions, because, at root, my only complaint with Morgenthau is that he neglected to tell fully the other side of the tale.

Second, the reader will discover only slight reference in this thesis to the phrase "conceptual framework." The thesis does not attempt to construct such a framework, although it will become obvious that both Kaplan and Morgenthau have. Thus this thesis is not predominantly constructive. It identifies the weaknesses of pure realism and empiricism, and points out pitfalls to be avoided in theory-building. Despite a rather comprehensive treatment of methodology in theory, it does not build a theory. Although it suggests at least a frame of mind which is a prerequisite for theory-building, it does not erect a framework or fill it in. It concludes that the realist and the empiricist both have tasks to perform in constructing a flexible framework by which to understand politics. Let us see, then, what the realist and the empiricist have to tell us about politics and, by seeing how they approach their conclusions, suggest a more fruitful direction for theory.

PART I

A CRITICAL COMPARISON OF KAPLAN AND MORGENTHAU

1. THE COMMON ROOTS OF PURE REALISM AND EMPIRICISM

The clearest way to arrive at an understanding of the extent to which the theories of pure realism as expounded by Hans Morgenthau, and empiricism as set out by Morton Kaplan, are alike is to trace what has happened to the so-called doctrine of the "natural harmony of interests," which finds its roots in the eighteenth century Enlightenment. Philosophers of that age were deeply impressed by the harmony of nature which the beginnings of modern science seemed to reveal. Could not this harmony also be found in the social structure, revealed in the nature of man and his institutions? Certainly these philosophers thought that it could. As soon as the "artificial" strictures which had been placed on political, economic and religious man by aristocratic governments, mercantilism and the Church were removed, the nature of man and his natural social institutions would be revealed. Then, the natural goodness of man operating within these institutions would assert itself. One might even say that the entire purpose of the Enlightenment as an

intellectual exercise was to explore the unfettered nature of man and his "true" institutions. David Hume thought it:

> worthwile to try if the science of man will not admit of the same accuracy which several parts of natural philosophy are found susceptible of. There seems to be all the reason in the world to imagine that it may be carried to the greatest degree of exactness. If, in examining several phenomena, we find that they resolve themselves into one common principle, and can trace this principle into another, we shall at last arrive at those few simple principles on which all the rest depend.[1]

The logical result of such a discovery, for Hume and for his contemporaries, would be the assertion of human nature into human dealings in such a way that a natural harmony of individual interests would result. In short: utopia.

Suffice it to say here that the doctrine of natural harmony did not succeed. Man must act as well as think. Nineteenth-century philosophers, failing to recognize the essentially dialectic character of such concepts as "freedom" and "justice," paved the way for nineteenth-century actors, who knew their dialectic character but felt constrained to act in spite of it, and pursued the Enlightenment perspective to the catastrophe at Sarajevo. Although the total inapplicability of the doctrine of natural harmony to the real world should have been recognized by 1918, the coming upon the world scene of the United States, in the form of utopian Wilsonianism, gave

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[1]David Hume, An Abstract of a Treatise of Human Nature (Hamden, Conn: Archon Books, 1965), p. 6.

the doctrine a second hearing. It failed this hearing at Geneva and at Munich. By 1939 the doctrine was, for operational purposes, a dead letter.[2]

As a result of the operational demise of the doctrine of natural harmony, the realist critique was born. Best expressed by E. H. Carr, this critique told us that natural harmony was a chimera; that it had never existed, and doubtless never would. Carr's critique was elaborated further by Hans J. Morgenthau, who replaced "natural harmony" with the judgment that "the idea of interest is indeed of the essence of politics and is unaffected by the circumstances of time and place."[3] In short, the traditional values of the Enlightenment perspective having been manifestly shown in error by the force of events, a conceptual revolution was needed to fill in the gap. The realist critique could destroy, but it was unable on its own to build a new structure out of the ashes which it had created. New theory was needed.

There was one area in which the Enlightenment perspective - so it seemed - had not been called into question either by the force of events or by the realist critique. This was the area of science itself. Social scientists saw that the political values of the eighteenth

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[2]E. H. Carr, The Twenty Years Crisis (London: MacMillan & Co., Ltd., 1961) Chapter 6.

[3]Hans J. Morgenthau, Politics Among Nations (New York: Alfred A. Knopf, 1961), p. 8.

and nineteenth centuries had been decimated by the realist critique, but "scientific truth" remained. The flaw of the Enlightenment, it was alleged, was in the fact that values had been postulated *a priori*, and political science had developed around them. "Many of the most influential political writings," observed Harold Lasswell and Abraham Kaplan, "- those of Plato, Locke, Rousseau, the *Federalist*, and others - have not been concerned with political inquiry at all, but with the justification of existent or proposed political structures."[4] Had not the realist critique proved that? Of course. Was not, then, the solution to be found in a separation of political inquiry from political structures - of philosophy from science?

With the blatant disregard that only a non-scientist can show for the real meaning of scientific truth and the scientific method, the sociological technicians fell upon the task of creating a value-free theory of social man. With complete ignorance of the extent to which Einstein and Planck had called into question the very roots of dependence on a classical scientific method, the technocrats set out to construct a world-view based, methodologically, on Newtonian concepts. Karl Mannheim showed the way. In order to determine which way history

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[4]Harold Lasswell and Abraham Kaplan, *Power and Society* (New Haven: Yale University Press, 1950), p. xi.

is moving we must discover *principia media* that are completely value-free.[5] Any objection that one's view of the past might be somewhat colored by one's view of the present was brushed aside with the rejoinder that "no one has as yet really understood the present from the past who did not approach the past with the will to understand the present."[6] Faith in science, then - and an outmoded concept of science at that - fulfilled for post-war social scientists the same function that faith in natural harmony had fulfilled for earlier philosophers and diplomats. In each case, the conceptual revolution was to usher in a millenium of peace.

Such are the intellectual underpinnings of the relevant utopia of Morton Kaplan. Moved by the devastation which the realist critique had wreaked on the values and concepts of the Enlightenment, Kaplan's scientific predecessors cast out all values (at least values expressed as such) and turned to description (political science) instead of promotion (political philosophy). The divorce of science and philosophy was thus rendered complete. It remained only for Kaplan to appear on the scene in 1957 with *System and Process* to bring the separation of political science and political philosophy to its logical conclusion. In his

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[5]Karl Mannheim, *Man and Society in an Age of Reconstruction* (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1940), Part IV, Chapters 4-6.

[6]*Ibid.*, p. 188.

chief work any vestiges of the philosophic elements of political theory which may have remained after the likes of Deutsch, Lasswell, and Snyder were cast aside. "This theory," asserts Kaplan on his first page, "may be viewed as an initial or introductory theory of international politics."[7] For Kaplan, political theory is necessarily scientific; elements of political theory which deal with values as values are non-scientific; therefore, by definition excluded from ever becoming a part of that theory.

The way in which the realist critique led both to the normless empiricism of Morton Kaplan and to the type of political theory stressing the role of power as a normative factor in itself (Morgenthau) will be discussed in the next section. What is important to keep in mind here is that the two have a common root: the realist critique. Even though it might seem, therefore, that Morgenthau's own critique of the scientific approach to theory building should discount the possibility of a common foundation for the two, it is clear that they have started in the same conceptual spot: by rejecting completely the values and concepts of eighteenth and nineteenth century political thought. The scientist is then moved to cast values aside completely; the pure realist to make "real" values - such as the national interest - normative.

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[7]Kaplan, op. cit., p. xii.

Actually, as will be shown later, the similarity of scientism and pure realism extends further than their common root. There's more of the realist power theory in System and Process and more of empirical science in Politics Among Nations than either author would care to admit.

It may be objected at this point that rejection of the assumption of rationality by Morgenthau precludes any but an accidental correspondence between his theory and that of a scientist. "Social problems," he avers in disparaging the scientists, "become mere scientific propositions which, like mathematical and physical problems, can all be solved rationally and with finality, once the right formula is discovered."[8] Does not Morgenthau's criticism of the latent utopianism of the scientific approaches place him at odds with Morton Kaplan? No, for two reasons.

First, although there is ambiguity here, it seems that Morgenthau's criticism of rationality is not directed at the conceptual stage of theory-building, but at the actual stage of value-implementation. In other words, the irrationality of which Morgenthau speaks is that of political man - not man as a builder of theories.

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[8]Hans J. Morgenthau, Scientific Man vs. Power Politics (Chicago: University of Chicago Press, 1946) pp. 27-8.

If Morgenthau thought that his own efforts at theory-building, exemplified in Politics Among Nations, were irrational, he would doubtless have left them unwritten. Therefore, the irrationality of the scientific approach for Morgenthau is not the fact that "formulas" are sought, but the fact that scientists think that social problems will be capable of solution "once the right formulas are discovered." The fault is not the quest for answers, but that formulas suggested by scientists are not useful. In this belief, Morgenthau is very closely allied with Kaplan, who also suggests utility as the chief - if not the only - reason for his attempt at a theory of international relations.[9]

The second reason that Morgenthau's criticism of scientific efforts at theory-building does not apply to Kaplan on grounds of rationality is the peculiar nature of Kaplan's theory itself. Criticism of the latent utopianism of an assumption of rationality is valid when it is applied to scientific theories which take one aspect of politics, such as communications in the case of Karl Deutsch, decision-making in the case of Snyder, or elite-analysis in the case of Lasswell, and attempt to view the entire world political spectrum in terms of that one aspect. This criticism is not fairly applied, however, to Morton Kaplan. The sweeping nature of the generalizations offered

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[9]Kaplan, op. cit., p. xii.

in System and Process is surpassed only by that offered in Morgenthau's own writings. In his book, Kaplan has attempted to survey the entire political spectrum capable of being explored by behavioral and other sciences. Insofar as it is possible for a scientist to be a world-view thinker, Kaplan fulfills the role. There is no latent utopianism in a work such as that of Kaplan which merely attempts to set forth - in the broadest possible terms - what is. There is only the monstrous danger of static bias and worship of the status quo which may result from premature closure of a conceptual gap thrown wide open by the realist critique. It is into this trap that both Kaplan and Morgenthau have fallen - together.

2. THE COMMON METHODS OF PURE REALISM AND EMPIRICISM

It was mentioned in the previous chapter that the realist critique has led to both the normless empiricism of Kaplan and the pure power theory of Hans J. Morgenthau. The purpose of this section is to explain how pure realism and empiricism utilize similar methods in reaching conclusions. It should first be stated, however, that there is no particular reason why the realist critique should lead to either the results exemplified by System and Process or Politics Among Nations. It should lead, perhaps, to a deeper understanding of the nature of the values which the Enlightenment sought to implement, particularly to stress the dialectic character of such concepts as freedom and justice, by insisting that modifiers be attached to them. There is no necessary connection between the realist critique and the adoption of either a method which insists that theory should be value-free, or one which relegates that theory to the lowest conceivable level of human interaction. If the beginning of understanding in international relations is an appreciation of the realist critique, a necessary second step is an ability on the part of the theorist to transcend that critique in order to extract what is good from the Enlightenment and cast aside what is not good. Hoffmann is quite correct when he asserts

that pure realism is a counsel of despair and empiricism a counsel of both laziness and despair.[10] He might also have added that pure realism - considered from the scholar's point of view - is also a counsel of laziness, in that it, like empiricism, seeks a simple solution to an extremely complicated problem.

The effect of the realist critique on the methods of Kaplan and Morgenthau would seem, at first glance, to be divisive. The latter, desiring to invert the Enlightenment, seeks a return to the prerationalist era. From the former we get the impression that any new theory is good theory. What similarity can there be here? The contention would seem to be that there is more of an attempt at scientific methodology in Morgenthau's realism and more of the pre-rationalist methodologist in Morton Kaplan than is generally recognized. Do they meet, as regards method?

First, with regard to the extent to which Morgenthau is a scientist, one must examine the way Morgenthau approaches his subject. Experimental data to test scientific propositions cannot be gained in a laboratory,[11] therefore, the scientist must turn to history

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[10]Stanley Hoffmann, Contemporary Theory in International Relations (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1960), pp. 185-6.

[11]Although attempts have been made along these lines by Harold Guetzkow of Northwestern University. See Guetzkow, Simulation in International Relations (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963).

for evidence. The reader familiar with the realist school of thought cannot fail to be impressed with the fact that Morgenthau differs from other realists in his conception of history. Carr goes to great lengths to warn his readers of the extent to which history is contingent on one's view of the present,[12] and Niebuhr states that "there are no simple recurrences in history and therefore no analogies between sequences in various periods of history which could compel us to accept a proposition that a given policy in a certain period will have similar effects as a social policy in another period."[13] In contradistinction to this, we find Morgenthau telling us that:

> ... if life in society were completely contingent and irregular; only religion and philosophy would be able to give meaning to the historic past. This is, indeed, the opinion of those who find that whatever meaning and order there is in history is only the reflection of the historian's own mind. Yet even to the contemporaneous observer, the contingencies of the present and of the future array themselves in a limited number of typical patterns.[14] (Emphasis supplied.)

The reason that Morgenthau differs from other realists in his conception of history is that he desires

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[12]E. H. Carr, What is History? (London: MacMillan & Co., Ltd., 1961).

[13]Reinhold Niebuhr, Christian Realism and Political Problems (New York: Charles Scribner's Sons, 1953), p. 84.

[14]Morgenthau, Scientific Man, pp. 149-150.

to make generalizations regarding the nature of international politics which will be universally applicable, and feels it necessary to use historical examples to support these generalizations. Thus, we find "whatever the ultimate aims of international politics, power is always the immediate aim" supported by "the Crusaders wanted to free the holy places from domination by the Infidels, Woodrow Wilson wanted to make the world safe for democracy,"[15] etc., etc. To this extent, certainly, Morgenthau can be classified as a "scientist."

It should be pointed out here that any theory of international relations must draw the preponderance of its evidence from history, and that generalizations concerning the nature and direction of international politics may be induced from this evidence. The writer does not dispute these facts. The danger of such an enterprise must, however, be taken into account. It is that an assumption may be made, and all "facts" of history handled in such a way that the assumption will be verified. This is the fallacy of premature closure. It is not truly inductive; hypotheses such as the animus dominandi tend to become self-generating conclusions. Morgenthau should have heeded the words of the "prerationalist" philosopher

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[15]Morgenthau, Politics among Nations, p. 27.

Thomas Aquinas:

> Explanation is of two kinds. One goes to the root of the matter, as in natural science when a sufficient proof is advanced to show that the velocity of astronomical motion is constant. The other is less radical, but lays down a hypothesis and shows that the observed effects are in accord with the supposition, as when astronomy employs a system of eccentrics and epicycles to justify our observations about the motions of heavenly bodies. It does not carry complete conviction because another hypothesis might also serve.[16]

If Morgenthau had heeded these words, he might have realized that the drive for power, around which his theory is built, "does not carry complete conviction" despite its objective truth. "Another hypothesis," such as a drive for community, or both a drive for power and a drive for community, "might also serve." By closing his system around the concept of the animus dominandi in order to build - like any scientist - an internally consistent, complete theory, Morgenthau paved the methodological way for the spate of empiricists who followed him, and whom he now disparages.

The way in which Morgenthau's methodology is related to that of Kaplan is now probably becoming clear to the reader, but the similarity will not be fully evident until the extent to which Kaplan is a pre-rational methodologist is discussed. Kaplan seeks to develop a scientific politics by treating the materials of politics in terms of system of action. A system of action is defined as a "set

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[16]Thomas Aquinas, Summa Theologica (Chicago: Encyclopedia Britannica, Inc., 1952), p. 177.

of variables so related, in contradistinction to its environment, that describable behavioral regularities characterize the internal relationships of the variables to each other and the external relationships of the set of individual variables to combinations of external variables."[17] Kaplan carries the concept of system to the field of international politics by describing six possible types of international systems and the processes which occur within these systems. He thus organizes his theory around the two concepts which his title suggests: system and process.

Whether Kaplan's theory is open or closed may best be determined by new evidence which does not at first fit in. Let us test one of his rules with new evidence. Kaplan avers that "in a loose bipolar system, the leading member of a bloc, if it is of the directive, subsystem-dominant type, will identify bloc and national interests."[18] Assume for the moment that the Soviet Union decided to annex Greenland, but did not attempt to enlist bloc support for this move. If this were to happen, Kaplan might qualify his hypothesis with the addition: "except when the interests of non-leading members of its bloc are involved in a policy move to such a small degree that enlistment of their support by the leading member would not prove worth the effort."

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[17] Kaplan, System and Process, p.4.

[18] Ibid., p. 63.

Perhaps instead he would describe the behavior of the Soviet Union as a "step-level function," which denotes a change in the characteristic behavior of an actor. If he adopted the first course, his method would appear to be static, in that changes to his hypotheses would not be handled in accordance with new "facts," but in accordance with the preconceived conceptual framework.

Adoption of the second course would seem at first to be another matter. "Step-level function" implies a dynamic character in Kaplan's theory which keeps it open. Indeed it would be an open system, were it not for the fact that classification of an "input" or "output" as step-level function or not is an entirely subjective matter, and that "characteristic behavior" described by Kaplan in 1957 is supposed to hold true for all political systems and processes for all time. What looks at first like a dynamic theory, then, turns out to be one capable of revision within, but completely incapable of being structurally revised by evidence which threatens the principal propositions (those setting up the six types of international systems and the regulatory-integrative and disintegrative processes) themselves. It is dynamic within, but static without: another closed system.

Let me illustrate this point with a somewhat homely example. Assume that a class were asked one multiple-choice question such as the following:

a. Tooth paste tastes good.
b. Tooth paste tastes bad.
c. Tooth paste doesn't taste at all.

Anyone answering this question would be tacitly granting the assumptions on which the question is based: first, that there is quality-taste; second, that there is a substance - tooth paste; third, and most important, that it is relevant to make a judgment concerning the quality of taste to the substance called tooth paste at this time. It would be inane to ask the question to the ancient Romans - they never had tooth paste. Likewise, an invention in the future might render tooth paste obsolete. To link this example to Morton Kaplan and make my analogy complete: at about the time it became completely irrelevant to answer my question about tooth paste, systems theorists would be coming out with detailed propositions like: "21% fewer cavities with Crest," and "Pepsodent whitens your teeth and brightens your smile." The retreat into abstract formalism makes the systems analyst say more and more about less and less.

The net methodological result of such an enterprise has been best described by Barrington Moore as "the new scholasticism" in social theory.[19] In other words the systems theorist, by enunciating propositions in a "systematic" manner, and by allowing changes to his subject-politics - to be introduced only insofar as they fit the preconceived methodological framework, has committed the same error of forcing its field to withdraw more and more

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[19]Barrington Moore, Jr., Political Power and Social Theory (Cambridge: Harvard University Press, 1958), pp. 89-110.

from reality as the scholastic philosophers of old - and modern - days. If this charge is applicable to Morton Kaplan - and Moore certainly feels that it is[20] - it requires no stretch of the imagination to see that Kaplan has answered Morgenthau's own call for a return to the prerationalist tradition, although certainly not in the way Morgenthau would have wanted!

The closure of theory of international relations around the first and second image of the animus dominandi in the case of Morgenthau, and the second and third images[21] of system and process in the case of Kaplan, then, has resulted from the pursuit of a similar method: one which postulates a hypothesis in the abstract, then examines all evidence in such a way that the hypothesis will be proved and supported. Their theories end conceptually in the sum total of international politics to one who agrees with them, and a relatively worthless intellectual exercise to one who does not. Since both Kaplan and Morgenthau obviously believe in their own theories, it should come as no surprise that the results which issue from their similar approaches are substantially the same.

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[20] Ibid., pp. 97-98.

[21] Kenneth Waltz, Man, the State and War (New York: Columbia University Press, 1959).

3. THE COMMON CONCLUSIONS OF PURE REALISM AND EMPIRICISM

Thus far it has been shown that pure realism and empiricism have a common conceptual root and use similar methods in their approaches to theory-building. It is the purpose of this chapter to elaborate on the similar conclusions reached by the approaches of pure realism and empiricism. Here again, as in previous chapters, Kaplan and Morgenthau "speak" for their respective schools.

It is axiomatic in the field of international relations that conclusions reached by theorists must avoid two dangers. The first is that of unduly argumentative prescriptions on which the great number of theorists are unable to agree. Most political theorists have come to the view that political theory must be more than a procession of polemics. There should be some sort of a "mainstream" of opinion on political matters from which scholars may branch out to form their particular ideas on particular issues.

The other danger to conclusions in political theory is that in reaching for this "mainstream," theorists may resolve their differences only by agreement on some sort of a "lowest-common denominator" which is almost universal in terms of its ability to produce agreement, but almost completely irrelevant in terms of its ability

to be applied to political action.

Political theory must avoid these twin pitfalls. Undoubtedly, the number of theories should be less than the number of theorists, but we must avoid purchasing agreement at the price of relevance. If there is to be a "common fund of propositions," it must be above the lowest-common-denominator level. It was understanding of this which led Karl Mannheim to call for the elaboration of principia media (freely - middle range principles) in social theory.[22]

To date, Mannheim's call for agreement on principles above the lowest-common-denominator level has not been answered. The next section will discuss the question of whether or not such agreement is ever practicable. Suffice it to say here that such agreement has not been reached. Accordingly, we find political theory drifting toward the two poles of excess subjectivism and egregious irrelevance. It is the contention of this thesis that as scientific theories, pure realism and empiricism tend toward the latter pole. We can agree on the presence of political "systems;" we can agree that men and states possess - or are possessed by - a drive for power. The point is that despite the insight we might gain by learning these facts, they do not, standing alone, teach us a great deal about politics.

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[22]Mannheim, op. cit., Chapters 4-6.

The concept which is utilized by both systems theory and power theory to produce agreement is a very old one in politics. It is the notion of equilibrium. Individuals and nation-states must act (Morgenthau tells us) and do act (Kaplan assures us) in such a way that an international equilibrium is induced.

The source of the compelling power of the concept of equilibrium to produce agreement among political scientists is the undisputed fact that international politics is conducted among nation-states. A system, Kaplan tells us, is composed of active elements. These elements settle in some sort of balance. This balance is an equilibrium - a balance between some nation-states on one side and some on the other. Morgenthau reaches exactly the same conclusion. The "struggle for power" can be allayed and the "struggle for peace" won only on the precondition that a balance of power between competing interests is achieved. It is all true. Any theorist who has passed through the realist critique with any understanding whatsoever will agree that in any but a one-power world, a "balance of power" - or an "equilibrium of active elements" (take your pick: they mean the same thing) - is a precondition for stable world order. Those theorists who doubt it after two world wars and twenty-two years of cold war may indeed be dismissed as "outside the mainstream" of contemporary political thought.

The point is that the concept of equilibrium is totally irrelevant without a modifier alluding to the type of equilibrium that is desirable. Here again, the field of theory becomes divided, polemical, and in no essential agreement. Some critics of Morgenthau point to an apparent contradiction between what he says in Politics among Nations and what he recommends in his essays. This writer would not be so inclined to label the alleged inconsistency a contradiction as to stipulate that Morgenthau has engaged in a "lyric leap" between his correct judgment that an equilibrium is necessary and his understanding of the form it should take and the policies best suited to result in any particular kind of equilibrium. One can find himself in substantial agreement with Politics among Nations and disagree completely with Morgenthau on specific issues. The conceptual flaw is the irrelevance of the concept of equilibrium standing alone.

The way in which Morgenthau reaches the necessity for equilibrium is very simple: since interest is of the essence of politics and since it is, as anyone knows, important to create a safe world, this world must be created by achieving a balance between competing interests (i.e. a balance of power). Furthermore, he says, "the balance of power and policies aiming at its preservation are not only inevitable but are an essential stabilizing factor in a society of sovereign nations."[23] By "inevitable"

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[23]Morgenthau, Politics among Nations, p. 167.

in this context, Morgenthau means that states must choose policies based on the balance-of-power principle, since that principle is the only conceivable foundation for a safe world. The balance of power, then, flows naturally from the *animus dominandi* as a necessary condition for a safe world.

Kaplan also reaches the conclusion of equilibrium from extension of his central concept: that of system. By analogizing the "system" concept from such organisms as the human body and such mechanisms as a thermostat to political systems, he reaches the conclusion that political systems tend toward a sort of equilibrium; stable, unstable, or "ultra-stable" (searching for a new level of stability).[24]

Although the central concepts of Morgenthau and Kaplan are different, both theorists arrive at the "goal" of equilibrium by essentially the same route: by postulating that an equilibrium (or, in extreme cases, an imperial domination by one power) is the best that one can hope for as a solution to world political problems. To be sure, equilibrium as a *terminus* follows logically from extension of both the exclusive concept of the drive for power and that of system. If all politics is power; if all political action is seen as an expression of the *animus dominandi*, then the best one can hope for is a tenuous, ever-shifting balance-of-power - a situation of equilibrium.

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[24]Kaplan, *System and Process*, p. 7.

If all political action is "systemic;" if international actors are at all times viewed as behaving in a system-oriented fashion, then we can never free ourselves from the necessity for an equilibrium within the international "system" itself - no matter which of Kaplan's six systems is the best description of world politics at any given time.

This writer does not contest the necessity for equilibrium either in the "power" sense or in the "system" sense. Between now and the arrival of the millennium (and it certainly appears that the millennium is not around the corner) a balance-of-power will be a necessary precondition of the resolution of the "struggle for power" and for victory in the "struggle for peace." This is simply hard fact which history since 1914 has drummed into our heads. What is contested here is the underlying assumption made by empiricists (and here it is understood that Morgenthau as well as Kaplan is an "empiricist") that the production of this balance-of-power - this system in equilibrium - is <u>itself</u> the millennium; that equilibrium is not only <u>good</u>, but <u>best</u>.

Moreover, promotion of the notion of equilibrium from the status of "necessary" to the status of "necessary and sufficient," is both conceptually unfruitful and politically unwise. It is conceptually unfruitful because it really doesn't tell us anything about politics. The concept of equilibrium, standing alone, is not a panacea but an ambibuity. Whose equilibrium, in the specific

political sense, are we to adopt? That of General Thomas Powers? Then let us hasten to bomb Communist China. That of Lyndon Johnson? Then let us continue (no pun intended) to bomb North Viet-Nam. That of Walter Lippmann? Then let us withdraw to coastal enclaves. That of Brezhnev and Kosygin? Then let us withdraw from South Viet-Nam entirely. There may be, in fact, a way in which these varying conceptions of equilibrium may be evaluated and chosen, but the concept of equilibrium itself will not do the choosing for us. "Equilibrium" standing alone is a dead end. Of itself, it cannot solve political problems, such as the U.S. problem in South Viet-Nam. To know that an equilibrium is necessary is instructive, but to know what type of equilibrium is desirable, and to know precisely how, in specific situations, to act toward the most desirable type of equilibrium is the very essence of political action. Any of the proposed "solutions" to the Viet-Nam problem would bring about a new equilibrium, but what kind of equilibrium do we want? How can we best achieve it? It is on the answer to <u>these</u> questions that political actions will be predicated, and not merely on our knowledge that some sort of "equilibrium" is a necessary goal.

The empiricist fails to recognize that an appreciation of the necessity of international equilibrium has not solved the entire problem of world politics for him. What is merely a useful conceptual tool - "equilibrium" becomes the end-product of his theory. Behind the facade which the concept of equilibrium provides, he proceeds to

draw the most argumentative and subjective policy conclusions, while passing off his world-view as "objective," because it is based on the "objective fact" of necessity for equilibrium in relations among states. It is as if a doctor suddenly realized that to remain alive, human beings should remain healthy, then proceeded to all sorts of bizarre and contradictory conclusions on what constitutes health, and on the best way to maintain health.

The empiricist has reached a correct conclusion: equilibrium is a precondition of international order. The conclusion, correct as it may be, is a primitive one, and leads directly to no policy recommendations which can claim to be objective or productive.

Having agreed on the goal of equilibrium as a solution to world problems, Kaplan and Morgenthau agree also on the means by which this goal must be pursued: the national interest. Man (Kenneth Waltz's "first image") is a "given" in political theory; the international milieu (the third image) is anarchic and certainly not prone to the third-image solution (world government). For both, then, the means for pursuit of system equilibrium is to be found in the second image (the nation-state).

Morgenthau defines the primary objective of diplomacy (the tool which must be used to preserve international peace) as "the promotion of the national interest by peaceful means."[25] In order that diplomacy may be

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[25]Morgenthau, Politics among Nations, p. 539.

effective, he lists "four fundamental rules" and "five prerequisites of compromise."[26] In other words, for Morgenthau, the best guarantee of equilibrium is an enlightened pursuit of the national interest, the "light" to be thrown by application of the rules which he - a scholar - has laid down.

Kaplan reaches almost exactly the same conclusion on the route to equilibrium. He dismisses those who would regard the national interest as subjective as affirming "that it includes values other than power," then informs his readers that "national interests are objective and there are as many national interests as there are national needs."[27] The objectivity of the national interest thus asserted, Kaplan proceeds to offer his rules for the conduct of diplomacy, in the form of an introduction to the theory of games.

In both cases, then, the conclusion is reached that equilibrium must be sought by pursuit of the national interest enlightened by, in the case of Morgenthau, application of the four rules and five prerequisites, and in the case of Kaplan, application of scientific game theory to political situations. The correspondence between the two extends even deeper than this, however. Observe the similarity of the following two passages intended by their authors for use

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[26] Ibid., pp. 561-8.

[27] Kaplan, System and Process, p. 151.

as guiding principles for conduct of foreign policy:

> ... political realism considers a rational foreign policy to be good foreign policy; for only a rational foreign policy minimizes risks and maximizes benefits and, hence, complies with both the moral precept of prudence and the political requirement of success.[28] (Morgenthau)

> ... A criterion of rationality in the game is given by the minimax principle formulated by von Neumann and Morgenstern. The minimax criterion guarantees players a minimum game value.[29] (Kaplan)

The similarity here is too striking to be accidental, and indeed it is not accidental, for both Kaplan and Morgenthau have elevated prudence to the position of a supreme virtue for the same reason: because it fits. The animus dominandi being the sole principle on which human behavior, and a fortiori national behavior is based, it follows that a policy based on minimum risk and maximum gain is the only rational foreign policy. The minimax criterion being the only one for which solutions are possible in the theory of games, it follows that the only rational course for a player to follow is one which utilizes that criterion. This is worship of the status quo epitomized. This is the theory of pure realism and the scientific theory of politics come full circle. The social wrongs which could issue from the pursuit of a gamesmanship such as that ascribed to by Kaplan and Morgenthau could someday produce an exegesis on the part of a future

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[28]Morgenthau, Politics among Nations, p. 8.

[29]Kaplan, System and Process, p. 175.

philosopher which would make Marx's criticism of bourgeois misdeeds look tame by comparison, and a social solution, reacting to such control and manipulation which would make the Bolshevik revolution and its aftermath appear child's play. Fortunately, despite what they claim for their theories, they have not said the last word in international politics.

PART II

METHOD IN THEORY

From the foregoing, it is evident that the attempt at building empirical theory, in the cases of Kaplan and Morgenthau, has been a failure. The half-truth in political theory can be as dangerous, and ultimately unjust, as no truth at all. This must not, however, be the end of this thesis. "We have been engaged in a wrecking operation," as Stanley Hoffmann says, "Nevertheless, need for conceptualization and theory remains."[1] This portion of our thesis will examine the ultimate question of this thesis: that of the possibility of a science of politics, in order to determine where empirical theories fail and where, if anywhere, they may be of use in political theory.

To say that the question of the possibility of a science of politics is primarily a logical and epistemological one, rather than a substantive one is almost a truism. Indeed, the failure of most empiricists to handle this question on its own terms, which are indeed philosophical rather than, strictly speaking, political, results in large measure from their positivistic belief that such

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[1] Hoffmann, op. cit., p. 171.

questions are sophistic. "Let us not concern ourselves with such trivia," they seem to be saying, "and get on with the job." The questions are left to philosophers to answer, while the process of political model-building goes on unaware, or at least unmindful of the fact that such a default must result not only in mortal methodological sins, but ultimately also in that old empiricist bugaboo, unbalanced division of labor in building the supreme scientific theory of politics. Floyd Matson speaks wisely on epistemology when he asserts that "when man is the subject, the proper understanding of science leads unmistakably to the science of understanding."[2]

The reason that epistemological questions have not been engaged in the post-war development of "scientific" political theory stems from the Enlightenment, just as the "solutions" proposed by such theories often bear a twinge of the simplicity which the Enlightenment postulated in its approach to the complex political problems of the eighteenth century. What affects both the Enlightenment and the post-war social technocrats is the burning desire for political reform. A "theory," or a "conceptual framework" is often followed by a spate of "policy studies" in which the theorist, with government or foundation assistance, attempts to apply his theory for purposes of policy direction.

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[2]Floyd W. Matson, The Broken Image: Man, Science and Society (New York: George Baazillier, 1964), p. 259.

The desire for political reform is not of itself a bad thing; to be sure, we are all tempted at one time or another to it. What is dangerous is the potential injustice implicit in carrying out a political theory which has not been thoroughly thought through to its philosophical foundation. Laissez-faire was, allegedly, a true theory, yet it generated the excesses which spawned Marx. As the previous chapter has pointed out, notions such as "system" and "equilibrium" are likewise capable of excess in application. At the very least, it would seem mandatory to ask at the outset, and to keep asking, whether our conclusions are really scientific and, if so, in what sense and to what extent. If there can be no absolute guarantee of truth in theory of international relations it follows that there can be no absolute guarantee of justice in the conduct of international policies.

One looks in vain through empirical works such as Kaplan's System and Process for an exposition of the logic and epistemology which should underlie the supposed science of politics. The "limited time available" and the "pressing nature of the problems that confront us" have themselves made these scientists believers. Kaplan's only discourse on method, contained in the seven-page Preface to System and Process, from a logical point-of-view, is simply unsatisfactory. It is left for the polemical critics of scientific method applied to social theory to assert that a science of politics, or anything like it, is simply not

possible. There is a debate between empiricists and other critics, but no effective dialogue. The critics have won the round by default.

There are some earlier works which do attack the central question of the possibility of a science of politics, and answer it in the affirmative. They emanate to a large extent from the euphoria of the twenties, when the "problems" did not seem to be so "pressing," and, one might add, when foundations did not finance as much social research as is the case today. One excellent example is George Catlin's The Science and Method of Politics.[3] In this book, Catlin devotes an entire chapter to "The Possibility of a Political Science," concluding that it is indeed a possibility, and a highly desirable goal. A detailed look at the results of his research should prove fruitful in determining the logical strengths and weaknesses of "science" in "politics."

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[3]George E. G. Catlin, The Science and Method of Politics (New York: Alfred A. Knopf, 1927).

1. THE GOAL OF A SCIENCE OF POLITICS

If a science of politics is ever to be possible, it must start from some sort of clear appreciation of what that science hopes to achieve in epistemological terms. Of course, a science must be applicable; that is, it must be capable of being applied to specific sets of circumstances with a view to producing certain results. But utility is most definitely not the first test of science. The first test is validity of the grounds on which applicability of science to specific situations is based. The scientist must show the epistemological basis on which he makes general inferences from particular instances. If all the flaws of the post-war development of social theory can be ascribed to any single factor, it is precisely the failure of empirical, particularly behavioral theorists to appreciate this distinction. "Science," for them, means simply "applicability." A theory is suggested-for instance: "Military dictatorships tend to alienate the masses of peasants in under-developed countries." This theory is then indiscriminately applied to Brazil, Ecuador, Taiwan, etc., and "verified." But is it verified? The grounds on which the theory is applied to different situations are not established. As a result, some people (at least the author of the theory) believe the hypothesis, and some do not.

Surely, the result is not "science."

In order to investigate the grounds on which scientific theories are applied, the epistemological questions must be asked. What is "science?" What am I looking for when I set out for "science?" In this particular respect, George Catlin's answer may be revealing:

> A science, here and throughout, is treated as a corpus of certain knowledge, and (as certain knowledge) valid prior to particular experiences and despite appearance to the contrary.[4]

The key words here are "certain" and "valid." What types of "certitude" and "validity" is he alluding to? If they must be absolute, then the quest for a scientific politics can be completely rejected. No man can act in the social milieu with absolute certitude that the result he plans will in fact ensue from his action. Although dogmatically stated hypotheses often appear as though they are based on absolute certitude, this writer seriously doubts that empiricists, if fully questioned, would make this claim for their theories, or postulate the necessity of absolute certitude as a goal for science.

What Catlin is speaking of, as the remainder of his chapter on "the possibility of a political science" makes clear, is something less than absolute certitude. One might call it "practical" certitude. Just what this means, in specific terms, and whether it is attainable,

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[4] Ibid., p. 92.

and to what extent, in a "scientific politics" will be discussed shortly. It remains here to point out that it is not "absolute certitude," and no amount of quantification of evidence can make it absolute. The only science which really requires absolute certitude is mathematics, and even this may be in doubt. However, of the fact that (using our present numbering system) two plus two equals four, I can be absolutely certain. No additional experiments are required to prove this to me, so long as the system remains the same. "Military dictatorships" are a different matter. Situations change; terms gain and lose relevancy; absolute certitude here is a practical impossibility.

The fact of unattainability of mathematical certainty in social theory is misused by both sides in the debate over empiricism. This is an important point. On the one hand, critics of empiricism make the claim that, since this is the case, a science of politics is an impossible task. This view is a straw man. Although this thesis will conclude that a "political science" is indeed impossible, most critics of science have obscured the terms of the debate by fixing their arguments around the fact of unattainability of mathematical precision. In the realm of physical science, there are a number of fields subordinate to mathematics in which absolute certainty is a practical impossibility. An excellent example is physics. Prior to Max Planck's quantum theory it was thought that Newtonian mechanics was capable of explaining all physical

phenomena regarding motion. The arrival of the quantum theory changed this view, indicating that absolute certitude in Newton's laws had been an error. Our certitude in Newton's laws was in fact practical, not absolute. It enabled us to explain all phenomena observed at that time. Yet no one, not even the most stalwart critic of empiricism, could deign to pronounce that physics is not a science. We were then practically certain that "a body at rest or in a state of uniform motion tends to remain at rest or in a state of uniform motion unless it is disturbed by an unbalanced force, because it explained all observed phenomena regarding motion. We should have been "practically," rather than "absolutely" certain.

The fact that "another hypothesis might also serve" to explain motion does not reduce this practical certitude, unless the other hypothesis should provide a better (i.e., more universally valid) explanation for the phoenomena observed. This is what Max Planck did in physics.

On the other hand, proponents of scientific method as applied to social science have also misused the argument of unattainability of mathematical precision. In their view, such unattainability gives license for accepting almost <u>any</u> hypothesis as "practically certain." This view is equally erroneous. Although this fact hardly requires demonstration, it is frequently overlooked by empiricists. Take the following example:

> Hypothesis I: The Cold War is the result of Soviet aggression.

Hypothesis II: The Cold War is the result of Western imperialism.

Hypothesis III: The Cold War is the result of friction ensuing from the inevitable clash of interests between the United States and the Soviet Union.

Which hypothesis is correct? Are any of them correct; that is, can I be certain of any of them, or of any other hypothesis which might "explain" the Cold War? Surely one of them, or one which has not been set forth, is better than the others, in that it explains more phenomena. The third seems to be of this type, because more phenomena are explainable with reference to it. But are all observed Cold War phenomena explainable with reference to it? If it is to meet the test even of practical certitude this must be the case, just as the appearance of phenomena under the electron microscope which are not comprehensible with respect to Newtonian physics must lead us to reject Newton's laws as plausible hypotheses in explanation.

The rejection of "absolute certitude" as an object of social science, then, has led empiricists to the wrong conclusion: that all that is necessary for "science" is to postulate hypotheses in the abstract, in the hope that one of them will be verified. Kaplan's work is the best, but definitely not the only example of this methodological bushwhacking in theory of international relations. Whatever may be the result, it is certainly not "science."

The foregoing does not, however, deny the possibility of a science of politics. The fact that results of

research in the post-war period have been disappointing does not make a science of politics impossible, although by this time one might have expected empiricists to question their faith in scientific method. Absolute certitude is not a requirement for science, regardless of the misuse which both sides have made of the fact of its unattainability. Practical certitude, and by this is meant a state of mind which assents to the fact that all phenomena observed are explained by an hypothesis, is a requirement for science, whether it be physical or political. It is around the question of the attainability of practical certitude in politics that the question of the possibility of a science of politics must revolve.

Having disposed of the false argument of absolute certitude as a necessary object for science, it remains to be seen whether practical certitude is obtainable in politics. If the object of science is truth, and if truth in science is described as practical certitude, we must investigate the question of the possibility of its attainment in social science, and specifically in politics. Are there any hypotheses extant which can explain all observed phenomena regarding politics or an aspect of politics? If not, are such hypotheses a possibility?

The posing of these questions themselves brings up the first problem which social scientists must face in their quest for a scientific politics. This problem is the necessity for <u>objectivity</u> in science; objectivity which

must be obtained even at the price of disturbing vested interests. That objectivity is a necessity and that vested interests will oppose objective science, even in natural sciences, is made very clear by one of the foremost advocates of science in politics, Daniel Lerner:

> The sociology of scientific knowledge makes enlightening history. The requirement of a public, explicit, common fund of propositions was established only against heavy opposition through the centuries from the powerful personnel of the magic and religious industries. The struggle for a rational science of medicine against those skilled in charms, amulets, incantations, herbs, and leeches is well remembered. So is the struggle for an observational science of astronomy against those skilled in court astrology and clerical cosmology.[5]

Nevertheless, the struggle against these vested interests must be faced. Objectivity, which connotes truth at whatever cost, is a key test of science. Catlin's argument in favor of a scientific politics points out the necessity for it:

> Economic science by its early utterances pleased the dominant class. Political science in its early utterances displeased the insurgent and victorious classes. And yet, unless politics ventures, as Economics ventured, to clear for itself the forest of detail by the use of abstract hypotheses and of a scientific method, it can no more hope to advance to the status of a science than chemistry could without the atomic theory.[6]

What Catlin was looking for by developing a science of politics, as what political scientists are

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[5]Daniel Lerner, Evidence and Inference (Chicago: Free Press of Glencoe, 1960), p. 7.

[6]Catlin, op. cit., p. 95.

looking for today, is some sort of a guarantee of justice in politics just as Keynesian economics, at the time Catlin wrote, was thought to contain a guarantee of economic justice. It is hoped that by being objective; that is, by approaching the evidence free of values (postulated a priori) objective "facts" may be found, that "causes" for these "facts" may be elicited, that eventually these "causes" will elicit "first principles" about politics, in the form of "abstract hypotheses." Morgenthau's power theory and Kaplan's systems theory are collections of such abstract hypotheses. These "first principles" having been discovered, political conduct will become a simple matter of application of the hypothesis to the situation. The key to this methodological approach is objectivity. As a first step, objectivity must be applied to the evidence at hand. The evidence at hand is contained in history.

Before the possibility of attaining objectivity in analysis of history is discussed, a short digression may be in order. This writer sincerely hopes that he has not overstated the hopes of the political scientist by a somewhat oversimplified view of the scientist's goal: a practically certain, value-free theory of man in his political milieu. It is admittedly true that many political scientists have restricted their horizons somewhat in the post-war period. Perhaps even the majority of books by empiricists since 1945 do not contain the sweep of Politics among Nations and System and Process. The reader will agree,

however, that the goal of the scientist, regardless of the content of any given book other than the two mentioned, has not changed since Catlin stated it in Science and Method. In a very real sense, which means in practice, the scientist in politics hopes to be able to deduce an "ought" from an "is," as the large number of policy studies emanating from the empirical community amply proves. This should be neither cause for embarrassment on the part of empiricists, nor mirth on the part of their critics. Their goal is indeed a laudable one. The thesis here is merely that it is impossible of attainment because the root from which it must spring (value-freedom) is a practical impossibility and the method it uses (hypotheses supported by empirical "evidence") does not carry the complete conviction which practical certitude requires of science.

2. THE ROLE OF HISTORY IN PROVIDING EVIDENCE

The key to objectivity in the social sciences is objectivity in the approach which the social scientist makes to his main body of evidence: history. In a sense, the only evidence available to both natural and social scientists is historical evidence. There can be no doubt, however, that the natural scientist has a distinct advantage in that if he does not believe the results of an experiment conducted in the past, he is at liberty to repeat it himself and thus confirm or refute a hypothesis suggested by, say, Newton or Einstein.

The social scientist has no such liberty. What's past is simply past, and he must rely on the record of history to provide him with facts so that the process of science may begin. Moreover, the record is entirely incomplete, and due to the unfortunate circumstance that the great majority of historical actors and historical recorders are dead, the record always will be incomplete.

Nonetheless, despite the fact that we will never be sure, for instance, why Plato wrote the Republic, there is plenty of "history" around. We may never be able, for instance, to completely eliminate the hypothesis that Peter Thanapopulous, of whom most of us have never heard, (he was an Athenian garbage-collector) forgot to show up at the

scholar's house one fateful day, and that the scholar, forced out of his house into the garden by the stench, began to walk around, and as he walked, he began to think ... No tears are shed for the permanent loss of enough "facts" to eliminate this hypothesis concerning the origin of the Republic. It is enough for most of us to know that Plato did indeed write it and too much for some. Most political theorists, whether well-disposed toward the empirical approach or critical of it, would probably be prepared to stipulate that the factual lynchpin of the proposed "scientific politics," if it is ever to be, is likely to be found in the portion of history that has been recorded.

The point we are making here is really a vital one. There is an important school of philosophical thought, headed by Karl R. Popper, which insists that recorded history is simply the history of "international crime and mass murder," i.e., the history of success, and that we will never have a scientific politics because someone neglected to fill in the tale with the history of struggles that failed.[7] Most political philosophers, myself included, would disagree. Although we might agree with Popper that recorded history is indeed the history of success, we would issue the rejoinder that most history in the political field has been recorded, and that our problems stem not

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[7]Karl R. Popper, The Open Society and its Enemies (Princeton, N.J.: Princeton University Press, 1963), II, pp. 270-1.

from having too few facts, but too many. Let us not weep over the grave of Peter Thanapopoleus, who wasn't even a good garbage collector, and accept the fact that Plato did indeed write the Republic, for whatever reason, and get on with the tasks at hand.

These tasks include selection of facts which are relevant, marshalling them in some sequence of cause and effect, and inducing from this sequence a few general hypotheses which, in their application to specific situations, will yield valid policy recommendations. The question is, is this possible in politics?

The first task being the selection of relevant historical "facts," we must first determine a criterion of relevancy. Since all facts are obviously not relevant, or since, at the very least, some facts are more relevant than others, some standard must be used to separate and categorize them. Different historians attach different shades of meaning to some facts, and completely disagree on the interpretation of others. One stalwart proponent of a science of politics, writing like Catlin from the innocent vantage-point of the twenties, alluded to this problem as follows:

> New knowledge must depend primarily on the phenomena selected for correlation and explanation. As soon as this is clear, it becomes imperative to agree on what the phenomena are. It is precisely the extent of this agreement which accounts for the degree of solidarity of thought in the natural

> sciences, and it is the lack of such agreement which so largely explains the rank confusion in the social sciences.[8]

It is doubtful that an empiricist would make the same sort of statement today. Empiricists have learned to cover their disagreement regarding criteria of relevance by further abstraction into such propositions as: "Systems and subsystems in the international systems have roles and these roles have different functions depending on whether they couple activity within the subsystems of a large system or between system levels;"[9] a statement which no serious student can even understand, much less oppose. The "science of politics" has not really advanced from the time Rueff and Catlin made their innocent call for a criterion of relevance to the time Kaplan published System and Process. Some political scientists have gone their separate ways, despairing of the hope of ever establishing a criterion of relevance for historical "facts," and engaging in polemics against the empiricists who have buried their disagreement on what the criterion is to be in agreement over such euphemistically meaningless propositions as the above. It is as if we were all agreed that "political actions involve human beings," as if such agreement could produce scientific results, or had any scientific (or political) relevance whatsoever.

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[8]Jacques Rueff, From the Physical to the Social Sciences (Baltimore: The Johns Hopkins Press, 1929), p. xxi.

[9]Kaplan, System and Process, p. 19.

The need for a criterion of relevance in the scientific approach to history remains. Toynbee calls the response of the Western democracies to Mussolini's invasion of Ethiopia, for instance, a "tale of sin and nemesis," basing his judgment on the "facts" of the existence of a collective security system, of the blatantly unjustified aggression of Italy, of the timid, spineless and opportunistic behavior of British and French political leaders.[10] Another earnest student (myself) calls the response "realistic" and "quite praiseworthy," basing his judgment on the facts of the European desire for peace at any price, the poor condition of the British Fleet, and the ambiguous position of the United States. Unless "sinful" conduct is "praiseworthy" there is a disagreement here on which facts are relevant. Whence does this disagreement stem, and is it insuperable?

Rueff was quite correct, albeit futile, in his call for agreement among political scientists on the selection of phenomena which are to be classified. Such agreement is a precondition of "science," properly understood. Withdrawal into "abstract formalism" by the use of such concepts as "system" does not eliminate disagreement, but plasters it over. "System," as the previous chapter has pointed out, is an ambivalent concept. It has

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[10]Arnold J. Toynbee, Survey of International Affairs, 1935 (London: Humphrey Milford, 1936), II, p. 1.

both wide and narrow connotations. If it is used in the narrow sense, its use will produce disagreement among political scientists regarding its relevance; used in the wide sense it merely covers disagreement, is analytically unfruitful and ultimately meaningless.

Nevertheless, despite the fact that Rueff's call for a criterion of relevance has gone largely unanswered among empiricists in the post-war period; despite the fact that such a criterion or criteria have not been agreed upon, the burden of proof is upon this writer to prove that the construction of such a criterion is an impossible task.

In effect, the search for a criterion of relevance is the search for similarities among historical phenomena. Raymond Aron has aptly phrased this in his statement that, indeed, "historical understanding consists of perceiving differences among similar phenomena and similarities among different ones."[11] It is almost pedestrian to point out, as so many critics of empiricism have, that no two historical situations are ever exactly the same. The fact remains that similarities do indeed exist, and political theory, to say nothing of political science, would be impossible without reference to these similarities. The Graeco-Bulgarian war was a war. World War II was also a

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[11]Raymond Aron, "Evidence and Inference in History," Lerner, op. cit., p. 27.

war. In this, at least, we can say that two events are similar. Without the conceptual tool "war," and without a number of conceptual tools like "war," political theory would be impossible. The political theorist finds in his history book a situation in which people under one flag were shooting at people under another flag, and calls it a "war." Whether that theorist is Thomas Hobbes or Morton Kaplan, nobody argues with him.

The disagreement begins when the political scientist begins to delve deeper into history in search of more similarities, so that his criterion of relevance may be established. One might say that the empiricist finds similarities too easily - that he is not critical enough to find differences where they are present, whereas the critics of empiricism are quick to note dissimilarities in historical "facts," and very skeptical of attempts to stretch "sameness" for purpose of analysis. In effect, the empiricist "finds" similarities because he wants to find them. The critic passes over similarities because he is not alert for them, and doesn't particularly care whether he finds them or not. Everyone agrees that there are both similarities and differences in history; the disagreement arises when the attempt is made to separate them.

In the foregoing I have purposefully overlooked one element of the "similarity-difference" debacle; namely, that politicians and sometimes even political theorists

frequently have a policy-axe to grind when they "find" historical precedents which are "made" to relate to contemporary world politics. We hear, for instance, how much the present position of the United States resembles that of Great Britain prior to Munich, with the obvious policy recommendation that appeasement be discarded as an alternative. Critics of U. S. foreign policy rejoin with "differences" between our present situation and the thirties, with the equally obvious recommendation that appeasement or at least "accommodation" not be rejected as an alternative. The reasons for this disagreement are not theoretical and academic, but practical. They are vital, however, in that they reveal a very important element in our argument: that political scientists must approach historical facts with some sort of stance - some perhaps incomplete theory, which they count on history to fill in. It is the necessity for this stance which makes final agreement on criteria of relevance an impossible task.

The world wars have made political scientists very skeptical of the notion that history is a neat arrangement of facts, with categories constructed in such a way that prejudice in approaching history is eliminated. E. H. Carr cites Lord Acton as having said in 1896 that "we can dispose of conventional history. ... now that all information is within reach, and every problem has become

capable of solution."[12] Almost a century of relative stability in international relations had lulled political theorists into seeing history as a flat field of facts, the "science of history" as a not-too-distant goal. As stability in international politics has become more and more rare, different philosophies of history have progressively diluted Acton's optimism, until we find Sir George Clark telling us in 1957 that "there is no objective historical truth."[13]

We may find Clark's pessimism a little extreme, but there is an element of necessary realism in it. We may set out, as Popper indeed sets out, to prove that "history" is merely the "history of international crime and mass murder,"[14] or as Marx did to prove that "history" is the "history of class struggles."[15] The important point is that these judgments, true or false - or half-true - as they may be, did not result from dispassionate, Acton-like research of a flat landscape of historical facts. Both Popper and Marx found in history generally what they set out to find. "Facts" were selected - as indeed they must be selected - on the basis of the stance which Marx

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[12]E. H. Carr, What is History? (London: MacMillan & Co., Ltd., 1961), p. 1.

[13]Ibid., p. 2.

[14]Popper, op. cit., p. 270.

[15]Karl Marx and Friedrich Engels, The Communist Manifesto (New York: Appleton-Century-Crofts, Inc., 1955), p. 9.

and Popper assumed toward the body of recorded history. If an appreciation of this does not make us subjectivists with Sir George Clark, it should lead us at least to a diminution of our layman's faith in the Acton theory of history. Empiricists, however, have not lost their faith in Acton's science, which plays for them the same role which faith in laissez-faire played for the political philosophers of the Enlightenment. Some "imponderables" must be overlooked - for the sake of science! The tail (science) begins to wag the dog (political theory).

The reason that different "stances" are taken toward the body of recorded history is that different histories are written by different people in different places and at different times. It is virtually inconceivable that Popper, for instance, could have seriously advanced - or for that matter even thought of - his "criminal" theory of history in 1896. It would have been contrary to almost all "facts" of nineteenth century world politics. War was not a crime, but "a continuation of diplomacy by other means," used to maintain stability in the European balance-of-power. If he had suggested his hypothesis in Victorian England he would have been an object of scorn and derision. It was World War I which made his theory relevant - if not quite tenable.

The "flat field of facts" simply does not exist. It takes a historian to breathe into the record of history and give it life, and as long as the world changes, there will be differing interpretations of what has happened in the past.

The empiricist fails to see the contingency of historical interpretation for what it is. Facts, for him, are facts. They are not to be re-interpreted, re-discussed, and re-lived in each generation, but to be fed into a machine which will decide on an interpretation which is timeless. But the very timelessness of the interpretation makes the science of politics static. Assume for a moment that the "science of politics" had gone into effect in the sixteenth century - around the time, for instance, of the Council of Trent. That Council was bound to have a vast effect on European social, political and religious life. The "machine" envisioned by the science of politics would be programmed in such a way that church councils would be given great weight in political affairs. What would this timeless machine tell us today of the probable effects of Vatican II? One can easily see that most of its predictions would be wrong, and almost all would be irrelevant.

The "science of politics" when confronted with the need for criteria of relevance in interpreting historical facts, then, can take one of two courses. It can withdraw into euphemisms, covering real disagreement and accomplishing nothing, or it can retreat into "timelessness," which glorifies the status quo. Both characteristics are present in modern political empiricism. Neither is useful, or politically relevant. "Objectivity," in the sense understood by modern empiricism, is simply not possible. Criteria of relevance on which we can have final agreement for the proposed science of politics do not exist, nor will

they ever be found. Historical "fact" is a relative concept.

If the above arguments are applicable to the quest for objectivity in approaching historical facts, they are even more applicable to the search for historical causes. Since it is impossible to pronounce final judgment on the interpretation of any given historical fact, it is at least equally impossible to do the same for the political actions which generate historical facts. It is not the intent of this writer to engage in the "free will-determinism" debate which has so long plagued the social sciences. We are perfectly willing to admit that any historical fact can be thought of as having had a determinate causal sequence which produced one result and could not have produced any other. The point is that this debate is an irrelevant one, for purposes of historical analysis, because the determinate sequence cannot, finally and objectively, be worked out.

Raymond Aron, in his essay on "evidence and inference in history" uses the immediate origins of the First World War as an example to prove the point under consideration.[16] Probably no historical events except those surrounding the life of Christ have ever been so thoroughly culled over with a view to determining causes as those which occurred between Sarajevo and the German invasion of Belgium. Agreement is far from complete.

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[16]Aron, op. cit., pp. 29-30.

Determining causes is fixing responsibility, and emotions become involved. The passage of time, and with it emotional involvement, may ease the quest for "causes" somewhat, but will never finally resolve it. We know the causal sequence is there; we may even know that it is historically determinate. What we don't know, with "scientific" assurance, is what it is.

The empiricist balks at such a revelation. Science must be "objective," and we seem to be suggesting the glorification of subjectivism. To be sure, subjectivism in the extreme carries with it the weight of a mighty injustice, and the danger of totalitarianism. But so, in the extreme stasis which ensues from objectivism, does its counterpart. The point is that an important - one might even say a crucial - element of political theory is to some extent "subjective," and that far from denying it, we should recognize it and work with it. The raw material of political science is man in his political milieu. No matter how the political scientist tries, he cannot free himself from his own humanity to view man with the objectivity that science requires. Nor should he try. The humanity of the social scientist is not a shackle to be broken but a gift to be utilized. We must agree with Louis Wirth, who tells us in the Preface to Karl Mannheim's most famous work that "insight may be regarded as the core of social knowledge," and that it is "participation in an activity that generates purpose, point of view, value,

meaning, and intelligibility as well as bias."[17] The shackles which church and state placed on scientific knowledge in previous centuries have been broken. It is time to take the emphasis off "objectivity" and place it on the wrongs which can ensue from a dogmatic objectivity run wild. Catlin tells us that knowledge resulting from the use of scientific method "may give us, as every practical science should, increased control, the control of men over the hitherto alarmingly uncontrollable behavior of men."[18] Are we to replace the shackles of church and autocratic state with the shackles of the technocrats? This is a question which our generation has not asked, but it must be asked and answered if political theory is to remain free of arbitrary strictures and ensuing injustice.

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[17]Louis Wirth, "Preface," Karl Mannheim, Ideology and Utopia (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1940), p. xxii.

[18]Catlin, op. cit., p. 107.

3. THE FUNCTION OF HYPOTHESES IN THEORY-BUILDING

A look at "historical fact" and "historical cause" has shown the impossibility of attaining the value-freedom which is a prerequisite for the science of politics. There is yet another reason for the impossibility of that science, however, and one which is dealt with at less length by political theorists, but is no less damaging. It concerns the method which the science of politics must use.

Any science uses a combination of inductive and deductive logic to arrive at conclusions. That is, a scientist observes a phenomenon, repeats it experimentally in his laboratory, if possible, and comes eventually to a general conclusion about it which leads him to believe that further repetition of the experiment is unnecessary. From the evidence, then, he "induces" a general statement concerning the phenomena. The actual inductive "step" is generally taken by advancing a tentative hypothesis which explains the phenomenon satisfactorily. From the general statement, the scientist is able to "deduce" further explanatory statements. The processes of "induction" and "deduction" are closely parallel to "generalization" and "prediction." The foregoing discussion of history has

demonstrated the extreme tenuousness of the grounds on which the first step of science - generalization - is based. If the generalization must be tentative, continually subject to further refinement, even, possibly, completely in error or in danger of becoming irrelevant, the predictions which are based on it must carry the same lack of conviction. Therefore, the political scientist is well advised to keep his hypotheses limited and to advance them with great care. The wider the generalization in political science, the greater the danger of error.

The danger results from an "inductive leap" which must be taken when a limited amount of evidence has given the political scientist "enough" conviction to formulate a general hypothesis. The evidence of history is never complete, whereas the need for hypotheses in order to "build" inductive theory remains. The flaw of the empiricist here is that he is not critical enough of his evidence; the result is a general hypothesis which does not convince - does not really carry the "practical certitude" spoken of earlier.

The reason complete conviction is not achieved is that there is a real and distinct - not merely logical - possibility that, as Aquinas told us, "another hypothesis might also serve."

It should be noted that Aquinas was not the last logician to advance the above argument. Raymond Aron has said essentially the same about the use of hypotheses in social science:

> An hypothesis is confirmed by the events it enables us to predict. It would be dangerous to say that it has been verified, because an hypothesis - a certain system of thought or a certain psychological mechanism - is not the only one on the basis of which inferences can be made.[19]

In making this statement, Aron was following not only Aquinas, but the eminent and respected logician Morris R. Cohen, who says, in speaking of the acceleration hypothesis:

> Nevertheless, the evidence for the acceleration hypothesis remains only probable. The hypothesis is only probable on the evidence because it is always logically possible to find some other hypothesis from which all the verified propositions are consequences.[20]

If this is true even of physics, how much more true is it of political science, where the possibility of "another hypothesis" is far greater!

This writer can find only one empiricist who has handled this logical argument against the science of politics on its own ground. It is, strangely enough, Morton Kaplan, who wrote in 1952:

> Cohen points out that "verification" has become a fetish that has been substituted for critical examination. He believed that an insufficient distinction had been made between "verification" and "confirmation." Any hypothesis that explains things is to a certain extent confirmed. If one believes that thunder occurs because Zeus shook his rod, every clap of thunder confirms the hypothesis. But, Cohen holds, such confirmation is not verification, for it does not

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[19]Aron, op. cit., p. 28.

[20]Morris R. Cohen and Ernest Nagel, An Introduction to Logic and Scientific Method (New York: Harcourt, Brace & Co., 1934), p. 205.

> exclude the possibility that some other hypothesis may explain the phenomena as well or better. It seems doubtful to the author that even a positivist would advance such an argument, for it is, in effect, the fallacy of arguing from the consequent to the antecedent. (Emphasis mine)[21]

Kaplan's reply is, of course, completely specious and unsatisfactory. Arguing from the possibility of a contrary hypothesis to the lack of complete conviction of a suggested hypothesis is definitely not arguing from consequent to antecedent. If I hold a paper bag which I have been told contains ten pieces of fruit and the first nine turn out to be oranges, I may hypothesize that I held a bag containing oranges. My hypothesis may indeed be wrong. The tenth may turn out to be an apple. Is Kaplan to label me a positivist for pointing this out to him? Far be it from this writer to shake Kaplan's faith in Zeus; suffice it only to say that his reply to Cohen is unsatisfactory.

As a matter of fact, Kaplan implicitly accepts Cohen's objection to the science of politics by turning to another technique: Stringing together "probable" hypotheses so that the evidence in favor of the general theory will be overwhelming. However, this too is logically unsatisfactory. It amounts to the same procedure as trying to create a surplus by stringing together a group of deficits. Kaplan is thinking about the multiplication of

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[21] Morton A. Kaplan, Some Elements of the Legal and of the Political Philosophy of Morris Cohen (Ann Arbor: University Microfilms, 1952), p. 22.

conviction which results from connecting hypotheses which are, say, 95% probable. This writer is concerned rather with the multiplication of error which results from connecting hypotheses which are 5% wrong.

At this point, the argument passes from one of logic to one of mathematics, in which this writer is not adequately schooled to speak with assurance. The use of "probable" hypotheses in series has been critized at length, however, by Morris Cohen. He tells us that:

> The relatively small number of observations that we generally have to deal with in the social sciences makes the application of probability curve a source of grave errors. For all the mathematical theories of probability refer only to infinite series (for which we substitute as a practical equivalent "the long run"). Where the number is small there is no assurance that we have eliminated the fallacy of selection. The mathematical error of applying a continuous curve to a discrete number of observations produces ludicrous results.[22]

The point is that no amount of manipulation and multiplication will make a wrong hypothesis right, or an irrelevant hypothesis relevant.

It has been stated previously that practical certitude is a requirement for science. The logical point that "another hypothesis may also serve" should not be construed as indicating that, because of this fact, practical certitude is impossible in politics. The point is that there is a quantitative difference in the assurance

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[22]Morris R. Cohen, Reason and Nature (Glencoe, Ill.: Free Press of Glencoe, 1953), p. 353.

with which a hypothesis may be "verified" in a science such as physics, and that with which one may be verified in politics. The fact that "another hypothesis might also serve" need not normally be an item of concern to the physicist, but should always be of concern to the political scientist. Is the effective control of the rural population in South Viet-Nam by the Viet Cong, for instance, the result of Vietnamese nationalism, or the clever use of terror tactics by communists, or the desire of the peasants for peace regardless of who controls the villages? These three views are hypotheses, and are to some extent mutually exclusive. Premature closure around any one of them will result in such a situation that "facts" which verify will be admitted, and "facts" which do not verify will be cast aside as "irrelevant."

If this is true of "middle-range conceptualizing," it is even more true of theories which take a world view, such as Morgenthau's power theory or Kaplan's systems theory. The animus dominandi and the "systems concept" do indeed explain some phenomena; those phenomena which are not explained by them are declared irrelevant. For Morgenthau politics is defined, in effect, as an activity in which power is exchanged. Other aspects of political relations are considered irrelevant. It "naturally" follows that all politics is power politics. For Kaplan, "system" is defined in terms of "desirable behavioral regularities"; all irregularities, or regularities which

are not "behavioral" or "describable" are irrelevant. But if all observed phenomena with respect to a given object, be it control of the South Vietnamese rural population or "world politics" are not explained by a hypothesis, practical certitude, and with it the science of politics, are impossible. The tentativeness of historical interpretation must cause us to reject hopes that any "world view" hypothesis will ever be adequate to explain all political phenomena; the limited relevance and scope of middle-range concepts such as, say, "nationalism" to explain all phenomena about, the nation-state. The affirmative science of politics is simply a practical impossibility.

CONCLUSIONS

A deep look into the contingency of historical interpretation and the frailty of scientific method should be, one would think, a chastening experience. Surprisingly, for most empiricists, this has not been the case. Instead of less science, they tell us, more science is needed, as if multiplication of data would both force objectivity on us and make scientific method more workable.

To be sure, emphasis on objectivity in social science is praiseworthy. Empiricists are intellectual inheritors of the Enlightenment tradition in their skepticism of arbitrary selection of values which must be imposed on a docile and unwilling community. But it is the thesis here that the reaction of empiricism to arbitrary selection and imposition is an over-reaction; that their quest for real objectivity in both conclusions and method is an impossible one, and that their approach can result only in either euphemistic theories which rapidly lose their relevance in our dynamic world, or in theories which in their extreme freeze the status quo, with inevitable injustice in their application. The over-reaction to subjectivism in political theory leads to another subjectivism, the

more reprehensible because its values are not the stated values of "monarchy" or "aristocracy" or even "democracy" but the submerged values of the scientist. Values there will be in political theory as long as man is a political being, it well behooves us to get them out in the open where they can be critically examined and chosen. "We evaluate as we breathe," says Stanley Hoffmann.[1] If this is true, values must be put back into political theory, and method made to correspond to them, not the reverse.

It is the misapplication of scientific method which has led the social scientist to believe that he can deduce an "ought" from an "is." But the empiricist sees only one side of the coin when he sees man as contingent, determinate, and a political means. Scientific method can tell us nothing of man as a creator of history, as a free agent with political ends. To see man in this latter aspect, and to be able to relate the two sides of political man, a more creative method is needed - one which possesses a characteristic which scientific method knows too little of - insight. If the method of insight is subjective, at least it is admittedly subjective and, as will be shown shortly, it can be braked from excesses by the use of scientific method. The important point is that the methodological tail must not wag the political dog.

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[1]Hoffmann, op. cit., p. 188.

Important beginnings have been made along these lines, showing at least that the art of political theory is not dead. No one would label such theorists as Niebuhr and Aron "scientists," yet there can be no doubt that they say a great deal about politics - a great deal more effectively than any "empiricists." This is because they speak of values in politics and are not afraid of being labelled "subjective." Both have extracted from realism one necessary element of political theory: an appreciation of the extent to which the achievement of a lasting harmony of competing interests is not possible. And both have rejected the conclusion which pure realism draws from this appreciation: that because of the impossibility of such an easy harmony, the solution to political theory is a facile identification of the national interest and the interests of mankind.

Practical certitude we can never have in political affairs, but political man must act. This is the paradox which the statesman must face, and the dilemma which must be investigated by political theory. It is in the gap between the practical requirement to act, which arises out of a situation, and the moral requirement to act wisely (that includes, with complete information) which arises out of man's status as a moral being, that politics really takes place. A consideration of this gap must take human values into account.

The danger which political theory faces in its consideration of values is that of arriving at lowest-common-denominators, just as power theory and systems theory are lowest-common-denominators. F.S.C. Northrop falls into this trap by attaching similar weight to all world cultures. The implication is that if we add up all the values of the different cultures of the world and divide by the number of cultures, we will somehow arrive at true values.[2] Surely this method is erroneous. But Northrop has at least made a start. He has started to arbitrate the dispute between empiricism and its polemical critics, and he recognizes the paradox of politics.

Despite the fact that the "science of politics" is an impossible task, there is still a role for science and empiricism in modern political theory. The excesses of empiricism can and must be avoided, but this does not mean that science and scientific method must be completely cast out.

Stanley Hoffmann suggests three uses for science in modern theory. "Empirical science helps us," he says, "not to decide what we should want, but to see how we could get what we want, what would be the implications of what we want, and even what we should not want."[3]

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[2]F.S.C. Northrop, The Logic of the Sciences and the Humanities (New York: Meridian Books, Inc., 1959), pp. 295-9.

[3]Hoffmann, op. cit. p. 187.

With regard to the first role of science: the neat division whereby the philosopher tells the scientist what he wants and the scientist advises on how to get it is a dangerous one, for two reasons. First, it accentuates the division which has already occurred between philosophers and political scientists. Second, the distinction between ends and means is nowhere near as clear-cut as a "philosopher-what, scientist-how" division of labor would tend to indicate. In conceptualization about international politics, as in the actual conduct of foreign policy, ends and means are woven together to such a degree as to render a division virtually impossible.

With regard to the other two roles for science suggested by Hoffmann, that of advising the political theorist of the implications of a suggested value, and particularly of warning the theorist of possible adverse effects which may issue from a proposed line of action, there is indeed a place for the scientist. We might learn through use of scientific method, for instance, that political stability is a precondition for disarmament, or that good political relations among states, conditions permitting, generally follow good economic relations. The caveat which must be entered, however, is that such advice is of a transitory nature, and that the interlacing of "scientific" propositions to form a complete theory inevitably results in a closed system incapable of dealing dynamically with a dynamic world.

One immediate advantage of these roles suggested for empiricism is that it places political "science" where it belongs: back in the Enlightenment saddle of criticism, rather than in the self-defeating position of trying to build affirmative political theory. The terms of the debate in theory of international relations have been obscured by the fact that the critical spirit has indeed been too subjective. If empiricism will accept its reduced tasks, it will act as an effective brake on development of overly subjective or over-idealistic theory.

Empiricism can act more effectively, as Cohen says, in the role of "negating" values set up by absolutistic and unhistorical systems than of suggesting them. The great advantage of scientific method is that it enable us to rule out hypotheses. If, for example, hypotheses A, B, and C are all possible, and B can be ruled out by the empiricist - even tentatively - political theory will be so much the better.

This, then, is the role of the scientist: to work with the philosopher instead of against him, to cast out reliance on a scientific method which is largely inapplicable to social theory, and cease hiding highly questionable philosophic theorems in "scientific" language. It is time for empiricists to turn away from the problems induced by their own systems and to work with political philosophers on problems which divide the world.

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